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TREATISE

ON

DISLOCATIONS OF THE SHOULDER,

BY

GEORGE O. JARVIS, M. D.

AUTHOR OF LECTURES ON FRACTURES AND DISLOCATIONS, &c. &c.

TOGETHER WITH

IMPORTANT CASES

ILLUSTRATING THE BENEFITS OF THE ADJUSTER.

COMPILED BY GEORGE KELLOGG, A. M.
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PREFACE.

The following article on Dislocations of the Shoulder, originally appeared in Vol. xxxix of the Boston Medical and Surgical Journal. The clear light in which the subject is presented by the writer; the original and strictly philosophical mode of reduction, which he so graphically describes; the truly practical character to which he has reduced the principles and treatment of Dislocations, cannot fail to convince the reader of his perfect familiarity with the topics which he discusses, and of his peculiar qualifications for the task he has undertaken.

The same inventive genius which designed and perfected the Instrument, the only instrument in the known world, by which such a process of reduction could be accomplished, has here continued the work in a profound investigation of the principles on which reduction is to be effected, and a fearless *expose* of the inadequacy if not the absurdity of all the common modes. Indeed, in the opinion of some of the most eminent surgeons, this article supplies a desideratum in the theory and practice of this department of surgery, and secures for its author the respect and gratitude of every individual engaged in its responsible duties, as well as of every friend of suffering humanity.

At the solicitation of many members of the profession, whose opinion I highly esteem, I have been induced, with the consent of the author, to offer to the surgeons of the United States a re-print of this learned and conclusive article, and to add to it a few of the remarkable cases which strikingly attest and exemplify the advantages—the indispensible necessity of the Adjuster. Being the sole manufacturer of that Instrument in America, the value and importance of which are so clearly proved in the following pages, I cannot but hope that while I am, in this way, doing what I can, to promote the usefulness and unfailing success of the whole body of practicing surgeons; they, in their turn, will not be regardless of the claims of humanity upon them, nor of their own professional honor, in these days of quackery, but will encourage, by their influence and example, the universal introduction among them of an apparatus so valuable.

GEORGE KELLOGG.

CASE OF

DISLOCATION OF THE SHOULDER,

OF FORTY-TWO DAYS STANDING,

REDUCED BY DR. JARVIS' ADJUSTER.

BY G. O. JARVIS, M. D.

THE above is selected from a number of cases, some of longer, some of shorter duration. This is one, however, which may very justly be regarded as a fair exponent of the whole, except so far, perhaps, as there may have been any reason to fear, that in *this* the axillary artery was implicated.

It is not, however, for the purpose of showing "what has been done," that this case is presented for publication; but it is to "show how it should be done." Nor is it that another instance may be given, where through much tribulation, or some not easy to be accounted-for, yet lucky effort of our own, our labors have happened to be crowned with success; but that what may be regarded as the *true*, the *only* true principles, those on which we can with some confidence rely, may be so clearly set forth that they shall, at once, become obvious to all who will give themselves the trouble to examine the subject.

A further object is to show (not only by this case as an example, but by the philosophy which the subject itself necessarily involves) that the pathology of the two stages of a luxation, *recent* and *old*, is as distinct, and that they require as distinct and different modes of treatment to reduce them, as the several stages of typhus fever differ from each other in their pathology, requiring such a diversity in the mode of

treatment as may best fulfil the various indications as they arise.

A still further object is, to show in the clearest manner what should be regarded as the *correct mode* of using the adjuster in old dislocations. The importance of this, I trust, will not be questioned, when it is considered that it would not be strange should more than one be found of the *hundreds* who now own the adjuster, who either do not understand its use, or do not, from some cause, choose to apply it correctly, even in this particular kind of dislocation; some perhaps, for want of time, others for want of attention, and others again because the instrument itself involves principles so entirely different from those which they have ever been in the habit of using (whether true or false, it matters not), that they cannot seem to comprehend anything which would appear to reflect on those "long-cherished virtues" of surgery, or which would in any way place in the back ground *their own peculiar* notions. For those, however, who *will* bestow their time, who *will* give their attention, and wish to understand its use, in such cases, for the sake of the good service which they may possibly thereby render in their professions and also for those who may wish to examine the philosophy of reduction conducted on those principles, that they also may have an opportunity to practice it, I have penned the short article. But especially have I done it, that I might call attention to this much-neglected department of surgery. No department having received so little attention, none having made so little progress, as what may very properly be called *osteal* surgery; while in fact, in point of real importance, both to the profession and the world, it is not surpassed by any, if indeed it has its equal.

It would be but an act of justice to notice, in this place, a case in many respects parallel to the one about to be related, which was reduced by the adjuster, and published in this

Journal under date of March 17th, 1847, in vol. xxxvi., p. 145. The operation was performed by Prof. J. F. May, M. D., of Columbia College, and reported by Robert King Stone, M. D. The operation reflects great credit on the surgeons who performed it; since Prof. May and his reporter appear first to have designed their plan (which certainly was very correct) and then to have carried it out as designed, with very great skill. We would refer the reader to that case, as well as to the one which we shall now relate, as fully corroborating the views which we are about to advance.

During a recent visit at the South, and while I was in Mobile, Ala., I was invited by Dr. Lewis, a resident physician there, to visit the Marine Hospital of that place, of which he was physician and surgeon, for the purpose of reducing a dislocation of the shoulder, of forty-two days standing. Accordingly, on the following morning, in company with Drs. Levert and Carter, of the same place, I proceeded to the Hospital to examine the case, and, if judged practicable, to reduce it. A very tall and rather spare man was shown to me, as the subject of the injury in question; yet, notwithstanding his leanness, it was obvious he had good general health, and possessed much muscular strength. I judged him to be about 45 years of age. On examining the shoulder, the head of the humerus was found resting in the axilla, partly under the pectoralis minor muscle anteriorly, while posteriorly the head rested on the axillary artery, so that there appeared to be nothing between the fingers and head of the bone, except the artery and common integuments. The artery was felt to pulsate strongly between the fingers and head of the bone, and was but slightly moveable on the head. The arm had acquired great freedom of motion, equal to what I have sometimes found in the same kind of dislocation, of at least six months standing. Attempts had previously been made in the Hospital to reduce it, but without any success.

Here some questions arose, with regard to the practicability of reduction; the most important of which was—Are there any adhesions formed between the head of the bone and the artery, which would endanger the life of the patient by reduction? Although, from some cause, the artery could be made to move but very sparingly on the head of the bone, still, from the uninterrupted current of blood constantly flowing through the artery, as could be determined by the *feel*, I was led to the opinion that there could not be formed any greatly condensed cellular tissue, uniting together the artery and head of the bone, so as to endanger life by reduction, provided such reduction could be accomplished without using violence to the parts around the joint. This opinion, I believe, was also entertained by all the gentlemen present. As it regarded the great *mobility* which the limb had acquired, although it was such, as had, with other causes, defeated all previous efforts, and would undoubtedly defy all the means in common use, still I was inclined to the opinion that by the adjuster a *momentum* might be communicated to the head of the bone, *by the muscles of the arm*, which would disengage the head, and re-open the capsule, through which, doubtless, it had escaped; and thus force it back to its normal position. On this point, however, none present could consistently express an opinion, having never seen the adjuster used, but all thought so favorably of it, as to desire that the instrument should be tried. Here a medical friend present (who, by-the-by, is quite clever in his profession) suggested that he could reduce it by his heel in the axilla, he having much confidence in that mode, especially since it had not been tried in the present instance. I therefore requested him to make the attempt. He, however, declined, saying he preferred to see me use the adjuster, rather than to have the dislocation reduced in any other way. I assured him that he would have that opportunity after he had made every effort in his power;

for that I did not believe it to be in the power of *any* man to reduce it, by any old or ordinary means. He still entertained the opinion, however, that it could be so reduced. I then insisted the more strongly on his making the attempt, assuring him, now that he entertained that opinion so decidedly, I could not consent to even apply the instrument until the effort was made. Seeing, probably, that I was determined on this, he accordingly made the attempt, calling to his aid just such individuals of those present, as he chose to assist him. Tart. antim. had been given the patient, and was occasionally repeated during the whole operation, but, if I recollect rightly, not so as to produce sensible nausea at any time. If I may be allowed to be any judge of such matters, I can assure the reader it was not "all boys' play." After having persisted in the effort (how long, I know not, but until all hands were well fatigued), it was again given up as a bad job; and the case was therefore handed to me to try the power of the adjuster.

The instrument being arranged, it was applied according to instructions given in Jarvis's Lectures, fourth edition, pp. 62, 63, 64. It was then stated, that in all probability, at least one full hour would be occupied in making extension, &c., before even an attempt would be made at reduction. Reasons for this will appear hereafter. Extension was now begun, and was increased slowly, from time to time, as the muscles were found to yield to the extending force of the instrument, until it was judged that the head of the humerus was extended fairly beyond the line corresponding to the plane of the glenoid cavity. We were thus occupied in applying the extending force, and in manipulating the limb, the more effectually to disengage the head of the bone, about one hour. It is at this point, then, at which we are to arrive, before even an *attempt* be made at reduction. It being

therefore judged that we had now reached that point, the effort was made in the following manner.

Dr. Levert, being requested to assist in the operation, placed a silk handkerchief high in the axilla; then seizing the two ends, he forcibly drew the head of the bone outward and a little upward. In this he used all his force to bring the head of the bone out from the axilla, while the arm and instrument was being carried forcibly downward and forward on the chest, so that the arm was made to press hard on the ribs. At this juncture the catch was raised from the ratchet wheel, so as instantly to let fly the whole force of the instrument; thus driving the head of the humerus forcibly upward by means of the *muscles of the arm*. On examining the shoulder, the dislocation was found not to be reduced. No time was, however, lost in applying again the same degree of force to the limb which had once carried the head beyond the plane of the glenoid; indeed, it was a little increased, since it could now be done very readily, and lest possibly we might have been mistaken before, in supposing the head to be beyond that plane, while in fact it was not. The arm was again carried forward and downward, as before, while Dr. Levert again assisted, as previously, in throwing the head of the bone outward and a little upward. The catch was again raised, and let fly the whole force of the instrument with the rapidity of lightning. On examining the shoulder this time, it was found to be "all right." All present appeared to be well pleased with the operation, and the result, and probably none more so than the patient.

I beg, here, to be allowed to comment briefly on the foregoing case. Ether was not used, and for the reason that it had not been employed in any previous attempt. Bleeding and the warm bath were not resorted to, and for the same reason. I was resolved, at the commencement, that it should

not be claimed that I had derived advantage from any of the ordinary means which had not already been employed. No doubt some advantage might have been obtained by employing either of them: although I confess I generally care not how strong the muscular contractions may be, provided the head of the bone can be carried beyond the plane of the glenoid without too much violence being used. The stronger they are, the more sure am I that the head, by this process, will be driven back through the capsule. Without which, reduction can never take place, in the state in which old dislocations are usually found.

From the little experience which I have had, I am convinced that there are hundreds of cases of dislocation now existing, which might, by the foregoing process, have been readily and safely reduced, though they may have defied at the time, all of the older and more common means, used *secundum artem.* To have persisted in them would even have cost the lives of many of the patients. I cannot but indulge the hope, that in the following remarks this fact will plainly appear.

I. *Reasons for adopting the foregoing mode to reduce old dislocations of the shoulder.*

In employing this mode, I refer to those cases only, where the head of the humerus lies below the glenoid cavity. This in fact, includes all, except two; one only of which is classed by writers on the subject among the ordinary dislocations of that joint. I mean that in which the head of the humerus is thrown forward against the corocoid process, resting on the margin of the glenoid cavity. In this, evidently the head of the bone does not lie below the glenoid, and of course the foregoing would be a wrong application of force. The other kind of dislocation to which I refer, (and only one case of the kind have I ever seen), is where the head of the humerus is thrown directly backward, resting on the neck of the scapula,

under and against the spine and acromion process of the scapula. I should, however, generally apprehend no great difficulty in reducing such a dislocation, provided force be applied in a right direction; but to apply it either in this or in the one just referred to, as it was in the case of the man at the Marine Hospital alluded to, would manifestly be a wrong application of power; while in all cases where the head lies below the glenoid, it would be a right application of that power.

Before entering fully into the reasons of which I am now speaking, it may be necessary to take a practical view of the anatomy of the shoulder: of its bony articulation, the texture of its ligaments, and the arrangement of its muscles, together with the particular *kind* of mechanical effect which these various parts may have on each other in their several abnormal relations. As to its articulation, then, suffice it to say, it is by ball and socket; the ball relatively very large to that portion of the socket made up of bone. Independent of that security which the muscles, proper to the shoulder, give to that articulation, by being attached above and below the joint, thus passing over it, together with the additional security which it derives from the atmospheric pressure which surrounds the joint, several *strong ligaments* are so disposed over it, as to admit of motion the most free of any joint in the body, and yet securely to tie the bones together, so as under no ordinary movements to be displaced by this great liberty of motion. The most important of these ligaments, especially in their relation to our present subject, is the *capsular*. It surrounds the joint; is attached in its whole circumference to the margin of the glenoid cavity; and being reflected over the head to the neck of the bone, is attached to it; surrounding, in like manner, both the head and the neck. Let us here notice a further fact in regard to this ligament. Its texture is *fibrous*, the fibres lying parallel, and running longitudinally over the joint.

We will now turn our attention to the *myology* of the shoulder. And in looking at that articulation we shall find it surrounded by twelve muscles, each of which is attached, at some point, above or beyond, and passing over it, is again attached at some point below the joint. We shall also find them distributed in the following order: three pass the joint on the top of the shoulder—to wit, the deltoid, the long head of the biceps, and supra-spinatus; six pass it on its posterior and inferior surface—to wit, the infra-spinatus, the sub-scapularis, the two teres, minor and major, the latissimus dorsi, and long head of the triceps; and three also pass it on its anterior and inferior surface—to wit, the pectoralis major, the coraco-brachialis, and the short head of the biceps. All of these twelve muscles, except the three first, viz., the deltoid, the long head of the biceps, and supra-spinatus, it will be seen, when the arm is a little thrown out from the body, and *traction* applied to it, tend to cast the head of the humerus downward, and to fix it below the centre of the glenoid cavity; and it is only while two, at least, of those muscles which pass on the top of the shoulder, viz., the deltoid and supra-spinatus, possess their full power to act, that the head, under the influence of such traction, would be likely to find the glenoid cavity, as the centre of those opposing forces. Let it be here observed, that in a dislocation of the shoulder, where the head lies below the glenoid, the long head of the biceps can have little or no effect mechanically on the head of the bone, to elevate it. This proceeds from the fact, that the head by being driven through the capsule, becomes disengaged from it; and the only influence which the tendon of the biceps can hold over the head of the bone, to elevate it being through the capsule, all of such influence necessarily becomes lost to the head of the bone. Under any circumstances, therefore, in such a dislocation, the deltoid and supra-spinatus muscles are the only ones which tend to elevate that head.

Now let us view these facts in reference to our practice. In all *complete* luxations of the shoulder, the capsular ligament must necessarily be ruptured or torn; and through this rent, must the head of the humerus escape from the glenoid cavity. The deltoid and supra-spinatus muscles are put largely on the stretch, and so must remain, ultimately losing all power to contract, becoming paralyzed by long-continued over distention, until the head is again restored to the glenoid cavity, when the tone of the muscles may be restored and the rent healed. That rent, be it observed, would, most likely, be a longitudinal one—since those fibres are more likely to be separated or torn asunder, than they are to be broken across; or, in other words, any round substance, as the head of the humerus, applied with force against the inside of this ligament, so as to rupture it, would be much more likely to make a longitudinal rent than to make a round hole. A moment's reflection will doubtless satisfy the anatomist of this; but should any one doubt it, let him try the experiment on the dead subject. This laceration may be a very free one, or it may be only sufficient to allow the head to escape; and, indeed, a little stretching may have been done even at that. Now suppose, in an adult but young and healthy subject, the head of the humerus should be thus driven through the capsule and lodged in the axilla, and it should there remain for five weeks or more. Within that period of time, by the reparative process which nature sets up, to repair as much as possible the injury done to the parts, the capsule has become healed, and the lacerated parts again united, except so far as the neck of the bone has served to keep them asunder. It thus becomes closely embraced by the capsule with the head on its outside. The deltoid and supra-spinatus muscles, have become paralyzed. Indeed, they are almost powerless, as may be known by their emaciated and withered form; while all the other muscles have

been able to retain nearly or quite their full vigor. Now what amount of force, let me ask, applied in the line of that limb, would re-rupture the capsule and return the head of the humerus again to the glenoid cavity? Is it not plain that force so applied scarcely tends to restore the head of the humerus to its cavity? My wonder is, how so many old dislocations of the shoulder have been reduced. By using such means, many, very many, have altogether failed in their attempts; and how they ever should have succeeded, it is not easy to divine, except we be allowed to suppose that the lacerated capsule had not become strongly united, and the deltoid and supra-spinatus muscles not completely paralyzed. Under such circumstances, I am fully aware, that, in addition to that degree of extension and counter-extension necessary to bring the head of the humerus out to a line with the plane of the glenoid, to apply force, outwardly, to the shaft, near the head of the bone, would be likely in many instances to effect reduction; *but not in any other instance.* It surely must be plain to every surgeon, at all acquainted with the anatomy of the shoulder, the character of its displacements, and the texture of the parts which surround it, that to depend for reduction on the application of force, in the line of the limb, with all the additional force applied transversely which one person could well command, while its capsular ligament is strong and entire (except so far as its lacerated edges are separated by the neck of the bone lying between them,) and the two muscles, the deltoid and supra-spinatus, remain powerless, would be but making an attempt, to be defeated in the end. The tendency of force thus acting in the line of the limb, is not, as we have seen in such a case, to elevate the head of the bone, but to depress it; and it is only by the force which we may be able to apply transversely, that we can even hope to succeed. And even in this, the greater the traction on the limb, the less must be

the influence of that force applied transversely on the head of the bone. With the ordinary strength, therefore, of a healthy capsule, we can hardly expect to succeed by such means.

To comprehend more fully the difficulties which attend reduction conducted in this wise, let us look for a moment at the effect which traction would have on the lacerated capsule; the head of the humerus being on the outside, and the laceration being, as we have already supposed lacerations generally to be, longitudinal. What would be the effect of an extending force, applied in the line of the limb, on the two lacerated edges of this capsule? Would it not be to approximate the two edges more closely, and thus tend to *prevent* the return of that head to the glenoid cavity? I think it would.

Inasmuch as it is now shown, I trust, that the *principle* which, in fact, all of the ordinary means involve, for reducing old luxations of the shoulder, is unphilosophical, is therefore incorrect, and consequently both the principle and the means employed are frequently inadequate to accomplish the end, it may not be amiss for me to show what I regard as the *true* principle, the *correct* mode, to be adopted, to reduce these old dislocations.

The true principle, then, is to avail ourselves of the contractile power of the muscles to reduce a dislocation; and thus, instead of having them our opponents, as they confessedly are, from beginning to end—our greatest obstacles in the way of reduction—to have them our adjuvants or helpers. This, however, has long been confessed, indeed held, to be the true principle, on which to conduct our efforts in reducing dislocations. But how has it been acted upon? Has it ever been so applied? Not to my knowledge. In using the ordinary means, what do we more, in any case, than to hold those powerful opponents (as in fact we make them to be)

at bay? We do nothing, absolutely nothing. If we can only so far overcome their contractions, as to give to the limb its full length, and there hold it during our pleasure, it is all we ever expect, or seem even to desire, of these mighty agents in reducing those old and grave luxations. This, certainly, is not using them to *reduce* the luxation; it is only using them, so as that we may by other means reduce it. And yet, after all that has been written and talked on the subject, this is the only way in which these most powerful agents have ever been employed to assist in overcoming this most formidable class of injuries. This, surely, "is talking one way and doing another."

We will, however, now attempt to show, that to adopt the method made use of at the Marine Hospital in Mobile, is *to employ the principle of using the power of the muscles of the dislocated limb, for the purpose of reducing the dislocation.* In that case, we made use of extension and counter-extension to elongate the arm, in order to bring the head of the humerus out to a line with the plane of the glenoid, *preparatory* to reduction; and also that the muscles might receive that degree of tension, which would give them force sufficient to reduce the dislocation. We also carried the arm downward and forward to the ribs, while our friend, by a handkerchief, forcibly drew the head of the bone outward. All this was *preparatory* to reduction. The humerus was thus made to point directly to the glenoid cavity; excepting that a *perfect* line of the bone would perhaps have carried the head a little outward from the cavity. And now, for the purpose of giving a momentum to the shaft of the bone, *by the muscles of the arm*, that the capsule might thus receive the *impetus of a blow*, from the head of the bone being forcibly driven against it, by those muscles, the catch of the instrument was raised, and thus the whole force was let fly in an instant. The head of the bone was driven through the

capsule, on the same principle that an arrow is driven into a board, except that in this the power was derived from muscles instead of an elastic bow. Thus was that dislocation reduced, entirely by the force of the muscles of that arm. And it was safely reduced, as every other instance has been where I have made the attempt in this way. *Here, then, is an example of a dislocation reduced by the muscles.* Indeed, there are many such. I must, however, beg leave to examine the subject still further.

II. *Reasons why the Pullies should not be used, to reduce old Dislocations of the Shoulder.*

1st. Because they operate from *fixed points*—thus, *of necessity*, confining the force substantially to one given line. That this is wrong, is obvious from the circumstance that force so applied can never be used to make the power of the muscles the *reducing* power; but must always, and in every stage of the operation, make them the opponents of reduction. And to overcome this resistance (if we succeed) we have the greater force to employ in some other way, which renders our success altogether more doubtful.

2d. Because force, so applied, tends to depress the head of the bone, rather than to elevate it, through every stage of the operation. And this tendency is overcome, only by another force acting transversely to the first. Indeed, force so applied, presents a strong impediment to the transverse force, just in proportion as they are opposing forces. This also makes reduction doubtful, because it tends to destroy the efficacy of the transverse force.

3d. Because force so applied cannot be a sure and ready way to re-rupture that strong, sero-fibrous membrane,—the capsular ligament, without which, the head of the bone can never be restored to the glenoid cavity.

For these reasons do I regard the action of the pullies, in reducing old dislocations of the shoulder (and I might add

many other luxations), as unphilosophical, and therefore incorrect. I doubt not that it is this uncertainty or want of success arising from the above causes, which brought them into such universal discredit in the days of J. L. Petit, nearly a century ago; and although in most parts of the world their use has to some extent been again revived, yet there is no part where they have been held in universal esteem, even to this day. There is none, so far as I know, where their uncertainty is not acknowledged. They appear not to have come into general use after their first introduction, by their inventor, Vetruvius, until the great Pare brought them into notice in 1582, after which, until the days of J. L. Petit in 1750, they obtained almost universal sway. Since that time, however, their reputation has been variable, uncertain, much according to the character of the company in which they are found, and, I doubt not, from the causes above indicated.

III. *Reasons why* PHYSICAL FORCE *should not be depended on, to reduce old Dislocations of the Shoulder.*

1st. Because, in many instances, it is like that of the pullies; except that it is not so steady, nor so uniform, and therefore not so much to be depended on, as even the pullies. But where the heel of the surgeon is placed in the axilla to be used as a fulcrum, for the humerus as the lever, the force employed is then unlike that of the pullies. It is not only more certain of success by the surgeon's holding at his command a greater force at the head of the bone, to act transversely; it is also more dangerous to the axillary artery, and cruel to the patient—two considerations which will ever govern the benevolent mind, in its conduct towards the afflicted. And although this may be regarded as the most certain of the two, still it is very far from being a reasonably sure mode, as every day's experience proves. Nor is it safe, or correct, so to apply force. Instances have occurred, in which, by this means, the axillary artery has been ruptured, and so in-

deed has it been by the pullies; yet by using the heel in the axilla, the danger must be increased nearly in proportion to the amount of force employed. I have heard tell (for I never saw it) of surgeons hanging their patients on the top of a door, and even of appending their own weight to the arm of the patient, and perhaps the weight of another person also to his heels. I have said to myself, so much for *his* mechanics, to say nothing of his knowledge of anatomy.

The physical force which we thus apply to produce traction on the arm, is necessarily attended with all the inefficiency, the unsteadiness, the uncertainty, which generally characterize manual effort, persisted in for a long time; and, therefore, if for no other reason, the greater force which the surgeon holds at command over the head of the bone, becomes useless to himself; and especially he does not in this way, in the least, use the power of the muscles, by which to accomplish his end. This alone (when they can be so used) is sufficient to reject that mode which forbids it.

IV. *Reasons why the Adjuster should be used, to reduce Dislocations of the Shoulder.*

In advocating the use of this instrument, I beg I may not be viewed with a jealous eye. I have simply related the process by which an operation was performed with it, and have given my reasons for it. I have told a simple tale, and the gentlemen herein named are my witnesses; and my apology is yet due to them, for thus bringing their names before the public, without their knowledge. But their known love of the profession, their high and honorable character as gentlemen, wishing to see justice done and truth advanced, are my apologies to them, for what I have written. If one word should be found, not perfectly agreeable to truth and their feelings, it will, with me, be a source of regret to my last breath. My object in this has been to benefit surgery. Indeed this was my object from the beginning, in arranging

and perfecting the instrument by which this and many other important operations have been performed. And now, after all the toil and expense which I have been obliged to incur, my chief consolation is, the conviction that if correctly used, it will *well* fulfil the ends for which it was designed; and, that if surgeons now fail, as heretofore, to accomplish those ends with elegance and skill, the fault is all their own, and not that of the instrument.

But to return from this digression. Our reasons for the use of the Adjuster, are the following.

1st. Because it is the only means, up to the present time, by which the *power of the muscles* can be so directed as to become, thereby, the reducing power. And also because, if need be, we can seize the proximal, instead of the distal, end of the bone, to reduce it. This last reason we cannot, at present, spare the time to prove by an example.

2d. Because, in consequence of its allowing this great liberty of motion to the limb, it thereby secures to the surgeon greater promise of success, while it is also less severe, and attended with less danger to the patient.

3d. Because the whole operation is completely under the direction, and at all times subject to the control, of the surgeon—he never requiring the aid of more than one assistant, and generally not even that.

4th. Because it furnishes the surgeon at all times with a means ready of application, and which can be just as conveniently and successfully used in the bed-chamber, in a ship, in the field, or indeed in any place where the person injured can be approached, as the means which are usually found can be applied in a well-furnished, well-regulated hospital, but far more effectively.

It now only remains to show, that the two stages of a dislocation, *recent* and *old*, are not only distinct from each other, when fully formed, but that they require different modes of

treatment to fulfil the indications which belong to each one in particular. From what has already been shown, while the subject has been under consideration, this *principle* (for it may be regarded as a principle) may be viewed almost in the light of a self-evident proposition ; requiring no argument to prove it. But, lest some should be found who still do not so consider it, we will devote a few lines to this part of our subject.

This division, then, is founded on changes which are well known to take place after every dislocation, provided such displacements be not soon reduced. They are chiefly the result of the laws of reparation in the animal economy, and of relaxation, as the consequence of over-distention of the muscles. Now it is obvious that these changes cannot take place without at once establishing a difference of pathological condition "wide as the poles." In the one case, we have only displacement, laceration of ligaments, and over-distention of muscles. In the other, we have displacement, the lacerated ligament as much as possible repaired, yet so as to confine the head of the bone securely in its abnormal position ; and, also, in addition to over-distention, we have relaxation, paralysis of some of the muscles and a rigid contraction of others. This difference, surely, is immense ; it is substantial, and may well be the basis on which to found the two stages spoken of above.

That this difference should, necessarily, require a difference also in treatment, will, I trust, no longer be questioned. The one, it will be seen, usually reqires little more than extention and counter-extention to reduce it; the other, much more. In it, extension and counter-extension can only be used as a means—a necessary means, to be sure—to *prepare* for reduction. The operation of reduction is all an after process. The examples given in the use of the adjuster, are fair illustrations of this fact.

A dislocation cannot, usually, in a strict sense, be said to be *old*, until it has existed full five weeks. All the impediments to reduction are not, commonly, fully formed before that period has elapsed; although, it is obvious, difficulties accumulate from the beginning, as a luxation is suffered to remain unreduced, but not frequently so that it cannot be reduced by the old and ordinary means. After that period, however, reduction, by them, for reasons already given, becomes very doubtful. It is my belief, from the little experience I have had, that difficulties do not increase after the above period of five weeks, as they do before. I have recently had reported to me a case of dislocation of the shoulder, which had existed for four years, which was reduced by the adjuster. It comes to me so well authenticated, that I cannot doubt it. Indeed, from what I had previously seen, I could not deny its probability—for the only question in my mind with regard to it, was,—Is the glenoid cavity so far obliterated by absorption, as to leave not a sufficient surface on which the head could be retained ? This question was with me at once answered, on re-calling to my recollection, a morbid specimen which was shown me in the London Hospital, by that excellent gentleman and accomplished surgeon, Mr. Luke. It was a specimen of morbid anatomy, in which, if I recollect rightly, as near as could be ascertained, the subject of it had his shoulder dislocated about eighteen years before death, and it had remained to his last hour unreduced. The head of the humerus was thrown forward of the glenoid cavity, and rested against the anterior margin of that cavity, a little under and against the corocoid process of the scapula. It had occupied that position so long, that by *attrition*, a deep fissure or fossa had been made in the head of the humerus, and a corresponding abrasion from the anterior margin of the cavity, against which the head had rested. But what was most singular, notwithstanding this constant wearing away of

parts, which came in contact, the other portions of the glenoid cavity appeared to be very little changed; so that, apparently had no portion of the glenoid margin been worn away by attrition, the glenoid cavity would have remained almost perfect, during the whole eighteen years of luxation.

I ought here, also, to state, that my friend showed me another specimen, which was in every respect the reverse of what has just been stated. This, too, was a specimen of a dislocated shoulder—which, if I recollect rightly, had taken place scarcely a year previous to death, certainly not exceeding a year and a half. But yet, there was remaining of the cavity only a very small portion; nearly the whole of it had been absorbed within the year, or eighteen months, and there was not remaining sufficient to retain the head of the humerus on its surface under any circumstances.

It may be difficult to account for such diversity in the process of absorption, in different cases; but from what I have observed, I have been led to the opinion, that this difference is chiefly dependent on the degree of inflammation which may succeed to each luxation. If, for instance, a dislocation is succeeded by great swelling, pain and inflammation, and this condition of the parts continues for a long time, absorption of a portion of the socket of that joint would most likely be a consequence of such a pathological condition; nor would it, very likely, be long delayed. But if, on the contrary, very little swelling, pain, or inflammation, succeed to such injury (and it frequently does so happen, as I have had opportunity to know), I see no good reason to expect that any material change would be likely soon to take place in the bony structure of that joint. In all *congenital* luxations, as far as I have had opportunity to observe them, the socket is never absorbed, although existing through a long life. This principle is so well settled, that M. Guerin, of Paris, told me that he had reduced a number of such disloca-

tions of the hip of many years standing; and one case I saw in his office, of a girl eight years of age, who had congenital luxation of both hips, and whom he had prepared by division of tendons for reduction. I regret not having been able to witness the operation, as I was obliged to leave Paris before the attempt was to be made: but I have since incidentally heard, that he succeeded also with that case. For the above reasons, I hold that many very old dislocations may be safely reduced. Indeed, I am convinced that entirely new rules should be speedily settled with regard to them, in the science of surgery.

TESTIMONIALS.

Copy of a Letter from Dr. Wm. W. Townsend, dated Chatham, Chester Co., Pa., July 8th, 1848.

Messrs. Kellogg,—Gentlemen,

I have had occasion to use the Instrument three times in Dislocations of the shoulder. The first was a patient of a neighboring Physician and skillful Surgeon; but he failed after repeated trials to reduce it, and with as powerful extension as could be made by three strong men. He then sent for me and I applied the apparatus in the presence of about 30 persons; and without any other assistance than the Adjuster, replaced or reduced it in five minutes.

The second case was of four years' standing. It is but justice to state, the patient has received a second injury,* breaking up the adhesions.

The third was a case from the Philadelphia, Pennsylvania Hospital, injured on the 9th of May, reduced on the 28th of June. I presume the reason it was not reduced in Philadelphia was owing to a fracture in the upper part of the Humerus.

I am satisfied so far as I have had occasion to use the Adjuster that it surpasses all articles or modes heretofore invented for accomplishing the ends which this one is intended to fulfil.

Respectfully,

Wm. W. Townsend.

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* What may have been gained by this "second injury" for the purpose of accomplishing reduction, must of course always remain unknown to us. But if the views advocated in the foregoing pages are in any measure correct, (and their accuracy we have never heard questioned,) it is indeed very probable that that dislocation was just as capable of reduction before as after the "second injury." It does not appear that the Capsule had been re-ruptured by it—for if it had, in all fair probability, the mishap itself, would have reduced the dislocation. There can be present no other body or substance, as every one must see, than the *head of the Humerus* by which to re-rupture the Capsule, and any force given to that head so as to have ruptured the Capsule by it, would almost necessarily have driven it through the Capsule, and thus have lodged it again in the glenoid cavity. It is therefore, we say, but fair to presume that whatever injury *other* parts about the joint may have received in the accident referred to, the *Capsule* was not lacerated or torn, and therefore the dislocation was substantially the same to reduce before as after the "second injury."

We are inclined to give more credit to the operator for skill in this operation than he appears disposed to take to himself. We think it, on many accounts, a very interesting and important case, and worthy to be registered in the annals of surgery, and one, too, which very justly reflects great credit on the operator.

Jarvis.

Copy of a Letter from Dr. Wm. S. Bracken, dated Harrisville, June 17th, 1848.

Messrs. Kellogg,—Gentlemen,

I received your communication of the sixth and eighth yesterday.

The case you desire me to report will have to be done from recollection. You will therefore not expect it minutely and in detail. The subject was Wm. Russell, Esq. a valuable citizen of our village. He is a very healthy, muscular man, in the prime of life, and weighing generally two hundred pounds.

[Here follows an account of the manner in which the accident happened, in which both the Tibia and Fibula were obliquely fractured, but which it is unnecessary here to detail, except that the accident happened about three miles from the home of the injured. Dr. B. then goes on to say:]

My neighbor, Dr. L. Howard, was first called, who dressed the limb in the usual manner, after which the subject was placed on a litter and removed to his residence in this village. I was then requested to attend with Dr. H. and dress the leg with the adjuster. I found on examination that notwithstanding Dr. H. had dressed the limb with judgment and care, it was about three quarters of an inch shorter than its normal length. I also learned from Dr. H. that he dressed it at its *full* length. We both came to the conclusion that the traction was owing to the imperfection of the apparatus used; by its not resisting with sufficient firmness the efforts of the muscles of the part. We therefore resolved to apply the adjuster. The double-inclined plane, arranged with suitable mats and bandages, was prepared for the limb, which after having been divested of all dressing, and the fracture-shoe secured to the foot, was placed on the plane. I now discovered what Dr. H. had before informed me, that the fracture of both Tibia and Fibula was oblique. That of the Tibia commencing in front, a little below the termination of the maleolus process and extending upwards and backwards, very obliquely. The limb, by this time, was very much tumefied. The dressings having been removed, the lower fragments were drawn full two inches above their normal place. The Belt was now secured (with its pads) to the head of the Tibia. The Instrument being placed on the inside of the leg and supported by the plane, the arm of the Rack was fixed in the loops of the fracture-shoe, and the cords with which the loops of the Belt were armed were secured to one arm (the other being removed) of the hinge-fork. We commenced making extension and counter-extension on the limb (which with this arrangement is a very simple process,) and notwithstanding muscular resistance was very great, yet by slow degrees the fragments were brought into co-aptation with very little increase of suffering to the patient. The limb was now supported with the necessary pads, splints and tapes, when the patient expressed himself as feeling more comfortable than he had expected under the circumstances. This comprises about all the history of that case except administering occasionally a sedative anodyne and occasionally, also, a laxative. Thus he rested comfortably well during the whole progress of the case. There was no

soaking with wet cloths, cold vinegar, or of Liniments of any kind. Two *contingencies*, however, are worthy of note—viz. On the third day he complained of *pressure* of the fracture-shoe on the heel. This was remedied by securing another Belt to the ankle, and with another set of cords carefully fixed in the line of extension. The point of pressure was thus changed without the slightest disturbance to, or variation of the line of the limb. An arrangement was thus established, too, which enabled us to alternate or divide the pressure at pleasure. About the eighth or ninth day, when tumefaction began to disappear, (and of course when inflammation was about established in the osseous tissue,) I discovered the Belt at the head of the Tibia had become slack or loose, and gave the pads liberty to ride on the Condyles. This was a difficulty I had not anticipated, and it of course for a time, gave me considerable uneasiness, knowing that co-aptation was maintained exclusively by the Instrument and that every thing depended on the firmness of my extending and counter-extending points; fortunately, however, I discovered the danger before the muscles had gained any thing and of course in time to apply the appropriate remedy of carefully tightening the Belt from time to time as required. The whole dressing however had tended to resist the contractile power of the muscles, so that no shortening of the limb had attained, and this, in some measure, secured us against such danger.

On the twenty-fourth day, an adhesion sufficiently strong to justify it, allowed us to remove the instrument and to depend on the support of splints. On the thirty-first day the limb was divested of all support save a roller bandage, and the Squire with a pair of crutches, took a walk down town! By enquiring of him since I received your letter, I learn that he could bear his weight on the broken leg on that day, but was unable to step on account of the soreness of the muscles—this, however, gradually disappeared, leaving the limb without deformity either in appearance, length, or position of the foot.

With regard to liberties afforded the patient they were striking. The flexed position of the limb enabled him to assume the sitting posture and by means of a rope extending from a hook in the ceiling, he was able and was allowed to assume that position at pleasure. By this means, and the support which the inclined plane under the knee gave the limb, and the liberty which he enjoyed of moving that joint without disturbing the fracture, he was enabled, in case of evacuating the bowels, to raise and seat himself comfortably on a vessel without any other assistance. He could be so bolstered as to recline on either side or on his back, and thus he actually attended to the practice of the magistracy during his confinement. Last, though not least, with one hand to the rope, the other supporting the upper end of the plain firmly against the under side of the thigh, and with the uninjured foot drawn up he could change his position in bed from side to side without the slightest disturbance of the injured parts.

I refrain from any comments. You have the case before you and can appreciate its merits as well as I can.

The case which I was obliged to treat while the above was in progress, and in which I could not have the benefits of the Instrument, it being already in use, was a dislocated ankle, in which the foot was turned outwards, the bottom

of the foot looking upwards and the articulating surfaces downwards, of course the ligaments of the joint were lacerated. I set some half a dozen fellows pulling at him. *You know how we formerly did it.* We slid it in, however, and I can hardly tell you how, only *we pulled and he hallooed.* The young man was about nineteen or twenty years of age, and resided in this neighborhood.

I should observe that Dr. Howard submitted Mr. Russell's case to my exclusive care, after the first dressing. I should be extremely glad to have you send me the plates, and if you have anything, at any time, new and valuable in your line, please let me know it. Wishing you much success in your valuable enterprise, permit me to sign myself your friend,

WM. S. BRACKEN.

Extract from the Gazette Des Hopitaux.

LUXATION OF THE FIRST PHALANX OF THE THUMB.

Dr. Michon, surgeon of the Hospital Cochin, has had the kindness to put at Dr. Jarvis's disposal, the two following cases:—

FIRST OBSERVATION.—No. 9, Salle Cochin, M——, cook, fell in the street, and throwing out his hands to save himself, received a compound luxation of the thumb. The accident occurred five days ago. During two days, various unsuccessful attempts were made to reduce the luxation. Extension was made by means of straps and several men. At the time of the operation, the first phalanx of the thumb was much swollen by erysipelatous inflammation; a large wound was found on the inner side of the thumb, near the dislocated articulation. The phalanx was dislocated outwards, and carried obliquely upon the second phalanx. To diminish the inflammation, leeches, poultices, and local baths of warm water, had been employed.

OPERATION.—The hand being placed upon the brass case, the fore-arm rested in a jointed fork joined to the counter-extending point. A strap fixed to the two branches of the fork, passed directly between the thumb and the indicator. A sailor's knot (made with a tape) attached the first phalanx to the extending point; this tape mounted very high upon the phalanx and compressed the wound. In consequence of the state of tumefaction, it made every thing attached to it slip off easily, and, in fact, prolonged the operation, the tape having slipped once during the extension.

The reduction was completed in about fifteen minutes.

SECOND OBSERVATION.—No. 11, Salle Cochin, Legendre, carter, has been confined to bed for twenty-two days* with a luxation of the bones of the metatarsus upon the tarsus. His horse fell upon him. Nothing has been done.

On the day of the operation, the foot was found to be swollen with a sub-acute inflammation, and was painful when handled; the internal edge of the foot presented a very concave line; the head of the first phalanx projected out, and a depression was felt behind it. The apophysis of the last phalanx very projecting over the external edge of the foot; the foot shortened and slightly twisted.

OPERATION.—The instrument being applied upon the sole of the foot, was fixed by bands which were carried around the ankle and the heel to the counter-extending point;

* I understood the Interne, five weeks.—G. O. J.

with other bands the foot was fastened above the inferior extremities of the bones of the metatarsus, and they were fixed to the extending point.

A stronger extension was here required than in the preceding case, but at the end of fifteen minutes, and by the aid of a pretty vigorous manipulatiou of the foot, the bones returned to their normal situation.

During the operation, the patient, although he was a very timid man, did not utter a cry.*

CASES TREATED BY JARVIS'S SURGICAL ADJUSTER.

From the Boston Medical and Surgical Journal of Aug. 23, 1848.

Case I.—The first opportunity which I had to test the powers and properties of Dr. Jarvis's Adjuster, was on the 18th February, 1847, in a case of dislocation of the hip, the head of the femur being thrown upwards and backwards on the dorsum of the ilium. The subject of the accident was a very stout Irishman, and about 25 years old—indeed, it would be difficult to find a more muscular man—and, withal, it was during the severe cold weather in the month of February, in which, and in the other cold months, dislocations, for obvious reasons, are more difficult to reduce than in the warmer seasons of the year. On the whole, I thought it as difficult and forbidding a case of reduction as any I ever saw. The accident occurred while the man was at work on a railroad, by his being caught under a slide of frozen earth.

I applied the adjuster, agreeably to instructions given in Dr. Jarvis's Lectures, fourth edition, pp. 34, 35, 36; and by a long-continued and judicious effort, I succeeded in thus reducing the dislocation. My patient recovered in the usual time.

Case II.—The second case in which I used the adjuster, and which was on the 6th March, 1847, was one of a very grave aspect.

Thomas Kerwin, a young man about 21 years of age, had had his thigh broken more than seven months previous to his calling on me. The limb, unfortunately, had been badly treated. On examination, I found it three inches shorter than the other, and very crooked, being bent outward at an angle of 25 degrees from its normal line. It had originally been an oblique fracture, and very near the centre of the bone. It was now firm, and surrounded by an ossific tumor, which, however, was largest on the inner convex portion of the bone. He had already consulted some of the best surgeons in this region of country, but they, regarding the case as one that could not be benefited, would not attempt any thing to improve the limb. I also advised him not to think of meddling with it, giving him, as the reason, that it would be attended with pain and danger to himself, and would be the occasion of much trouble to the surgeon who should attempt to improve it. And yet he might or might not be successful, even without danger attending it. His reply to me was, that he could not think of being a cripple for life, and if I did not engage for him, he would go to some one who would, &c. &c.

Finding him so determined, I at length concluded to make the best effort in my power to restore the limb to usefulness. Accordingly I called on Dr. Crandall to assist me in the case. We visited him, by appointment, on the 6th March, and found him unchanged in his purpose of having the trial made. We therefore placed him on a bench arranged for the purpose, and applied the adjuster in the way and manner directed for dislocations

* Nor did the man in the first operation.—G. O. J.

of the hip, as in the first case. After putting upon the limb the full force of the instrument, I gave a slight blow on the most convex portion of the bone. It gave way in an instant, and immediately assumed a straight position.

He fainted and fell back on the bench. After recovering from the shock, he was placed in bed and the limb dressed on the double inclined plane, in the manner directed in Jarvis's Lectures, pp. 47, 48 and 49, for oblique fractures of the thigh, keeping up constantly a moderate degree of extension on the limb, and increasing that extension daily as the patient was found able to bear it, until the limb became as long as the other. He was confined only eight or ten weeks, when he was discharged, cured, with a limb in every respect as good as the other, much to the joy of his parents and friends, and untold benefit to himself.

I have used Dr. Jarvis's Adjuster in many and various cases, and from my experience with it, I believe that in the hands of those who well understand its application, its powers and its properties, there cannot be found, for reducing luxations and retaining fractures, so good an apparatus.

Bethel, Vt., 18*th July*, 1848. ALFRED PAIGE, M. D.

This is to certify that I assisted Dr. Paige in the above case of Thomas Kerwin, in breaking up and re-adjusting a very bad fracture of Mr. K.'s limb—that the above case is described without the least exaggeration, and that he also succeeded in making a perfect limb of one which was extremely forbidding—indeed, almost without hope of a remedy.

Stockbridge, 18*th July*, 1848. HIRAM CRANDALL, M. D.

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